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Interactive Comment

Interactive comment on "Seasonality and extent of extratropical TST derived from in-situ CO measurements during SPURT" *by* P. Hoor et al.

Anonymous Referee #1

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This manuscript presents an analysis of in situ trace gas measurements from the SPURT airborne campaigns. The main objective of the paper is to characterize the extent and seasonality of the troposphere to stratosphere transport in the extratropics. The scientific issues discussed are very relevant to the scope of ACP. The authors present a novel dataset, using methodology extended from their previous publications [Fischer et al., 2000; Hoor et al., 2002]. A major conclusion of the paper, "the mixing layer above the local tropopause is strongly coupled to the extratropical troposphere", depends on the choice of 2 PVU level as the local tropopause, which is not supported by their data. I cannot recommend publishing at this point, but I strongly encourage the authors to revisit their analysis and resolved the issues detailed below.

Specific comments:

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Interactive Discussion
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1. Validity of the conclusion:"strong coupling of lowermost stratosphere mixing layer with the extratropical troposphere". As stated in the general comment above, I consider this conclusion to be questionable and not supported by their data. I give two main reasons for this statement:

1) Their conclusion disagrees with previous results. The seasonality of ozone in the extratropical tropopause region has been characterized by Logan [1999] using radiosonde data. As shown in Figure 8 of Logan [1999], the ozone seasonal cycle shifts from a Summer maximum (June-July) in the upper troposphere (1 km below the thermal tropopause) to a Spring maximum (March) in the lower stratosphere (2 km above the thermal tropopause). The conclusion drawn is that the seasonal cycles of ozone in the upper troposphere and lower stratosphere are distinct. The transition occurs at the thermal tropopause. The authors first need to reference Jennifer Logan's paper and acknowledge that their analysis and conclusion disagree with Logan. Second, they need to present evidence that their conclusion is correct and Logan's is wrong.

2) Their conclusion is not supported by the data shown in this paper. The CO and CO2 profiles presented in Figure 4 both show a marked gradient change around 25 K above the alleged local tropopause (chosen as the level of PV = 2 PVU), but show no gradient change at all at the alleged local tropopause (the altitude of delta theta =0). The authors' interpretation is that there is a mixing layer above the local tropopause and that the extent of the mixing layer is about 20-25 K in theta coordinates. My questions are, if the data show that there is a "mixing" or transport barrier at the delta theta about 25 K but no barrier at all at delta theta about 0, why should we consider the latter to be the tropopause but not the former? Furthermore, if the upper extent of the mixing layer is identified by the kink in the profiles, what physical evidence allows the authers identify the lower boundary as the delta theta = 0? An alternative interpretation of the data is that the kink is actually showing the transition between the upper troposphere and lowermost stratosphere and indicating where the tropopause is. This interpretation implies that 2 PVU level is not the right level for the tropopause, at least for the data

ACPD
4, S592–S595, 2004

collected on the SPURT flights. This is not inconsistent with related studies showing that on average the PV value of the thermal tropopause is about 3.5 PVU [Hoerlings et al., 1991].

A relatively easy test the authors can perform is to use the onboard temperature measurements during the vertical profiling parts of the flight legs (there appear to have at least 3 profiles on each campaign, Figure 2) to derive the thermal tropopause and do a comparison. If the kink in the tracer profiles are very close to the thermal tropopause and the level of thermal tropopause is around 25 K above the level of 2 PVU, the dataset would provide new evidence that supports the seasonal cycle in the tropopause region given by Logan.

2. If the authors can put the definition of the mixing layer on a solid ground, the extent and seasonality need to be shown more quantitatively. The current presentation using Figure 6 is not quantitative enough. It will be helpful to see scattered plot or statistics of CO (subTS) in equivalent latitudinal bins and how that change with season.

3. Furthermore, the quantitative statements of extent and seasonality need to be included in the abstract, since these are the main topic of the paper, as stated in the title.

4. Two clarity issues:

1) "Stratospheric end points" in last paragraph of p.1703 need to be described in more details. Since the first mention of the trajectory calculations is given in the overview section, the start of this part seems too sudden. It would be helpful to state more clearly what model is used (3D? isentropic?) and what is referred to as the beginning and the end of the trajectory in terms of time. For example, it will be helpful to state that the beginning points are the position and time the air parcels sampled by the aircraft, and the end points are the position of these parcel 10 days ago. Also, it is not clear if the term "stratospheric end points" means "these are positions 10 days ago for all parcels sampled in the stratosphere" Or "the positions of parcels found in stratosphere

ACPD

4, S592–S595, 2004

Interactive Comment

10 days ago". The paragraph is very confusing right now.

2) Derivation of upper tropospheric CO in page 1704 needs to be presented more clearly. The meaning of the content in the parenthesis between lines 7 and 8 in page 1704 is not clear.

5. Additional references required. In addition to Logan [1999] for the ozone seasonal cycle, the seasonal cycle and extent of TST as observed in the lowermost stratospheric water vapor has been discussed by Pan et al. [2000]. This work is relevant to the discussions in the first and second paragraphs of p.1694, and to the last paragraph of p.1705.

Additional References cited:

Hoerling, M. P., T. K. Schaack, and A. J. Lenzen, Global objective tropopause analysis, Mon. Weather Rev., 119, 1816-1831, 1991.

Logan, J., An analysis of ozonesonde data for the troposphere: Recommendations for testing 3-D models and development of a gridded climatology for tropospheric ozone, J. Geophys. Res., 104(D13), 16,115-16,150, 1999.

Pan, L., E. Hintsa, E. Stone, E. Weinstock, and W. Randel, The seasonal cycle of water vapor and saturation vapor mixing ratio in the extratropical lowermost stratosphere, J. Geophys. Res., 105(D21), 26,519-26,530, 2000.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 1691, 2004.

ACPD

4, S592–S595, 2004

Interactive Comment